T. C. Nelson —

Comments on PhD thesis

June 13, 1950

Dear Josh,

Thanks for reading the stuff in record time. I'm glad you agree with the main body in content but I await a careful reading with trepidation. Yes, I agree with you on your comments and suggestions. However I am not my own master on this deal - Ryan has to be cut in too. Any changes that are made in what is now the final draft of the thesis, that is, what you have a copy of, will have to meet Ryan's approval. I've already sent the business to Genetics. Actually I should have gotten your impressions first but I was fighting against time - getting it out before Ryan left and fulfilling the requirements for the degree so I could take the Cal Tech fellowship.

May I show your comments to Ryan? This will take time since he is now on the high seas. However we have a schedule of where he will be. If any changes are to be made I'll have to argue them out with Ryan.

Therefore I'll await your answer about talking over your comments with Ryan (by mail) before I start any detailed discussion of them with you. However I certainly agree with:

(a) Kinetic evidence is never sufficient as proof of a hypothesis, only as disproof, therefore this kinetic stuff is not crucial. Alternative quantiative formulations for transformation are myriad.

(b) So far ten different strains have been used and where protos are formed in sufficient number (no close linkage, no plate inhibition), in 6 different crosses (barring of course what are really repetitions such as Y 24 X 679-680 and Y 24 X Y 53) a good fit is obtained. So the results look fairly general.

(c) long math development
discussion of syntrophy and single revession
discussion of linkage
reference to MGB
all are Ryan's requirements - the MGB he could be talked out of probably

(d) collision factor for Fig. 11 assuming the bacteria follow diffusion laws:

number of collisions/fusion = 5,300 Fig. 11

from Schmoluchowski and Sutherland-Einstein

I've tried the Robinow stain after concentration by settling or by light centrifugation (where there are 300,000 protos in 10 to the 9th parents) and get the most unusual looking forms (chains, etc.) which do not fall into simple categories - a spectrum of unusual forms is given. This happens in A X A and B X B controls as well as the A X B experimentals so there is no refernce (as would be expected if no intratype matings occur but there is no reason to suspect that only intertype matings can occur). Agglutination is something that appears to happen here too.

Thanks again for your interest. Maybe we can bull it over more completely in California. I'm leaving next week but won't get into Madison but will drop off at Berkeley. If youre not around then we can clear this up by mail. I'll certainly expect to see Esther and you at Cal Tech.

This place is slowly plugging along - nothing new. Kim is, at last report, busy watching the antics of jellyfish at Woods Hole.

Sincerely,

Tom Wilson